bank was particularly vigorous; at such a place there might be a good many or a few or but one; the fewer, the bigger and better developed. They grew downward toward the sea from the cloud base in all cases.

FIG. 1.—Cumulus cloud with whirling mist filament, *x*, seen at entrance to Gulf of California, July 23, 1915.

I had a chance to see one very well. It lay like the arrow in figure 1, about which I have drawn a spiral line. (Fig. 1, *x*.) With the glass I could see the whirling filaments of mist, but they were not dense enough so that I could tell on which side they were, the near or the far, and so determine the direction of the rotation. The pattern was something like that shown in figure 2.

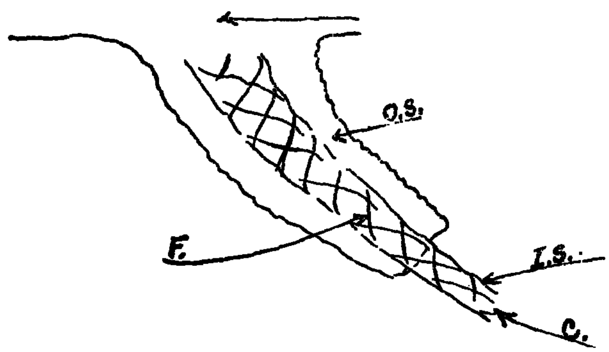

FIG. 2.—Details of the whirling mist filament of figure 1: Upper arrow shows westward direction of motion of the cumulus. *OS*, the outer less dense sheath, growing downward and following the inner sheath, *IS*. The external outline of *OS* was quite vague. *IS*, inner dense sheath, growing downward more rapidly than *OS*. *C*, the hollow empty core. *F*, mist filaments whirling and ascending, seen one through the other so as to give a lattice pattern.

Some of the funnels were so near the rear edge of the cloud as to be illuminated by sunshine; most were in shade, particularly the one above sketched was. The variations in form were from that of "A" to that of "B" in figure 3. Incipient forms were mere protuberances on the cloud base.

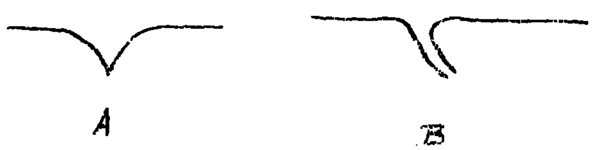

FIG. 3.—Limiting forms of whirls under cumulus clouds. A, seen from ahead of the cloud; very large, and reaching one-fourth of the vertical distance to the waves.

These two cloud banks seemed to join in a great cumulus bank far to the south.

July 24, 1915:

$\phi=24°12'$ N., $\lambda=112°14'$ W., at noon.

The dotted lines in the first figure were drawn from a comparison of the motion of mist wreaths in the clouds and the way the various tornado funnels lay below the clouds; they could not, of course, be drawn from direct observation.

I also saw a perfectly developed waterspout in the neighborhood of San Salvador [$?\phi=0°$, $\lambda=91°$ W.?].

CIRCULATION AND TEMPERATURE OF THE ATMOSPHERE.

By WILLIAM HENRY DINES, B. A., F. R. S.

[Dated: Meteorological Office Observatory, Benson, Wallingford, England, Nov. 1, 1915; received Nov. 17, 1915.]

Meteorology has made great progress during recent years and many of the ordinary phenomena connected therewith have met with simple and satisfactory explanations, but it must be confessed that the circulation of the atmosphere, the basis on which all meteorology depends, remains more or less a hopeless puzzle.

The circulation is due to unequal cooling and heating of different parts of the earth's surface, about that there can be no doubt whatever, the difficulty comes in as soon as we try to see what should be the natural result of this cooling and heating.

Disregarding for the present the local circulation, the moving cyclones and anticyclones, the facts to be explained are the trade winds, the high-pressure belts lying poleward of the trades, the strong westerly winds of temperate latitudes and the low pressures on their poleward side.

It might seem, at first sight, as though a mathematical solution might be obtained, but the difficulties are very great. It is hardly likely that any solution could be satisfactory which did not take account of the humidity of the air, since the latent heat set free by the formation of rain is enormous, and when the humidity and the viscosity are added to the difficulties due to the friction of the earth's surface and the disturbance due to the earth's rotation, the difficulties seem to render a solution hopeless. Notwithstanding, it is a matter of regret that of the few men gifted with very exceptional mathematical ability who appear from time to time, none have made a special study of the subject. While full mathematical treatment is at present impossible certain elementary mechanical principles are fundamental, however, and must be understood before any intelligent discussion of the problem can be commenced. For the elucidation of these principles meteorologists are greatly indebted to Ferrel who, in my opinion, has done more for theoretical meteorology than any one else.

In considering the circulation of the atmosphere the first point that meets us is the effect of the earth's rotation upon a moving body, in this case upon the moving air. In one sense the effect is small, but the cumulative effect is very great. The following seems the simplest way of stating the effect. A body moving freely is subject to a continual change in its direction—in the Northern Hemisphere it turns to the right, in the Southern to the left. It does not matter which way it is moving, east or west, north or south, and it does not matter, within the limits of ordinary wind velocities, how fast it is moving; if moving freely, its direction of motion will gradually turn to the right [in the Northern Hemisphere]. The amount of change or the deviation is proportional to the time and to the sine of the latitude, and the change of direction in one hour is given in degrees by the expression $15 \sin \phi$. Thus in latitude 45°, since $\sin 45°=0.71$ the change per hour is $10\frac{1}{2}°$, and a little under 9 hours, suffices to turn, in the Northern Hemisphere, an east wind into a south, or in the Southern Hemisphere into a north wind.

Suppose then that a place, *A*, lies north of place *B*, and that for some reason the barometer at *B* is lower than at

A. The natural result is that air flows from *A* to *B*, that is to say, there is a north wind blowing over the country between *A* and *B*, but in nine hours' time the earth rotation has shifted this north wind into an east wind. But as soon as any east appears in the wind, it is ceasing to move freely, since the pressure gradient is turning it back to the south, and as soon as it becomes a true east wind the pressure gradient exactly opposes the tendency to turn to the right, and we find in practice a steady east wind blowing across the line *AB*.

In recent years it has become a common practice to observe the upper winds by means of pilot balloons and it is a fact well established by observation that, within the limits given by the probable errors of observation, the wind at a height of one or two thousand feet is always blowing at right angles to the pressure gradient and that its velocity is proportional to the steepness of the gradient and has the value that is theoretically required. Now it is perfectly easy to explain the trade winds on this principle, but the irony of the situation is that it would be still easier to explain strong east winds in temperate latitudes on the same principle, but unfortunately for the explanation it is strong west winds and not east winds that prevail.

The explanation of the northeast trades commonly given is this. The sun standing vertically over the Equator makes the Equator hotter than the region of 20° or so north latitude, the warm air rises by virtue of its warmth and less density, the cold air from the north flows in to take its place [1] and this flow of air from the north or north wind is turned by the tendency of the earth's rotation into an east wind, and hence the northeast trade wind. The explanation seems perfectly good, no fault can be found with it, and it has been universally accepted.

But now vary the locality and reason as follows: The air over 40° north latitude is much warmer than over 60° north latitude, therefore it rises and cold air flows in from 60° north to take its place, inasmuch too as the temperature fall from 60° to 40° north is much greater than that from 20° to 0°, the resulting flow is stronger, and this flow being turned by the earth's rotation into an east wind produces the still stronger east winds of the temperate latitudes. There seems no flaw in the reasoning, but the conclusion is in flat contradiction to the facts.

I can offer no explanation but put the difficulty to those who are prepared to think out the question for themselves without reference to or bias in favor of current explanations.

But while the cause of the westerly winds remains unknown, they do follow as a direct corollary to the easterly trades. This rests on the fundamental mechanical principle of the conservation of angular momentum, it was pointed out by Ferrel and deserves to be more generally recognized than it is.

Taking the earth's atmosphere as a system by itself, its angular momentum remains practically constant from year to year about the earth's axis; therefore the total couple due to all the forces acting upon it must be zero. The internal forces have no effect upon the system as a whole, and the only external force is that due to friction with the earth's surface; hence the total moment caused by friction between the air and earth, that is, the total moment caused by the friction due to wind, is zero. The friction acts in the opposite direction to the wind, and since the total moment is zero any easterly wind in one part must be compensated by a westerly wind elsewhere. Moreover, since the leverage is greater near the Equator because the distance from the earth's axis is greater the equatorial winds must either cover a smaller area or be of less strength. The area is not smaller, and hence the winds of temperate latitudes are the stronger. It follows also from this that since angular momentum is transmitted from the equatorial to the temperate regions there must also be an interchange of air, since the momentum can be carried in no other way. Hence the circulations in tropical and temperate parts are not independent of each other.

There seems to be no particular reason why the winds known as the "trades" should not be westerly and the winds of temperate latitudes easterly. Perhaps such a system is possible and might be stable if once established. It would explain the glaciation of northwestern Europe, for it would very greatly lower the temperature of that region, but it is not feasible as an explanation of the glacial epoch because it would raise the winter temperature of North America. The prevailing winds and the distribution of pressure are so closely connected that if one is known the other is also known. According to Buy-Ballot's law, a person standing with his back to the wind has, in the Northern Hemisphere, the higher barometric pressure on his right hand and the lower on his left. Now, latitude 30° lies to the north of the trade winds and to the south of the westerly winds, and accordingly the barometric pressure in the neighborhood of the 30° latitude circle—either north or south—is high. Again latitude 60° lies on the poleward side of the westerly winds and its barometric pressure is low. The question naturally arises, does the pressure distribution produce the wind or does the wind cause the pressure distribution, and the only answer yet available is that we do not know, though the latter conclusion is the more probable.

It has been stated that latitude 30° shows a high pressure. Broadly this is correct, but reference to a chart will show that patches of permanent high pressure appear on the oceans a little to the westward of the large continents, especially in the summer. The anticyclonic areas in these regions are so plainly marked that they can not be due to chance, and any satisfactory theory of the circulation will have to account for them.

Areas of high and low pressure have been explained in a simple way. Broadly it is true that over a large continent the pressure falls in summer and rises in the winter. In summer the air is warm, rarer, and therefore light; in winter cold, denser, and therefore heavy. This is the explanation given. In the Azores barometric maximum in July the air certainly is not cold. Over Europe in summer, if the summer is one of exceptional heat like that of 1911, the barometer is exceptionally high, not exceptionally low as the theory would require. Moreover, observations made in the higher strata of the atmosphere have shown beyond the possibility of dispute that as the ordinary changes of pressure pass over the weather chart it is the cyclone that shows the low temperature of the air as a whole and the anticyclone that shows the high temperature.

The temperature of the air must, however, play an important part in the circulation. It is certain that were there no temperature differences there would be no wind, and therefore it is important to consider the distribution of temperature in the atmosphere. The question is greatly complicated by the fact that air is compressible and that its temperature changes as its vol-

[1] It is quite usual to say "warm air rises and cold air flows in to takes its place"; but this expression disregards the important dynamic side of the phenomenon. It would be more exact to say always, that the surrounding denser air pushes the warmer, lighter air out of its way or upward because of the constantly acting force of gravity. The warmed, lighter air or gas does not rise of its own accord any more than does the balance pan carrying the lesser load.—C. A., jr.

ume changes. Everyone knows this who uses a bicycle pump, for the end of the pump when the air is compressed becomes quite hot. In just the same way air that has come down from above is inevitably warmed, not because it has come down—that idea is a common mistake—but simply because it has come to a place where it is more squeezed and its volume is reduced. Conversely air that passes from a higher to a lower pressure is cooled because it has expanded. It makes no difference if it has risen 1,000 feet and thereby lost 1 inch of pressure, or if it has passed from the circumference to the center of a West Indian hurricane and similarly lost 1 inch of pressure. In both cases the loss of temperature due to expansion will be 5° F., but in the latter case the air will arrive at the center with the same temperature as the sea over which it has passed, as it will have been compensated for its loss at the expense of the sea. The rising air can obtain no compensation, as it is in contact only with other air subject to the same conditions as itself and in consequence it will have cooled nearly 5°. This adiabatic heating and cooling has very important effects upon the distribution of temperature in the atmosphere and upon the circulation, because it enables warmer air to lie under cooler. If two liquids of different densities are put in the same vessel, wine and water, for example, in a tumbler, it is impossible to make the lighter lie under the heavier, though the converse condition is easily obtained. Similarly if some water at a temperature of 40°F. be colored and placed in a glass with some clean water at some other temperature, the colored water can be placed at the bottom and the clean water, if it be inserted without mixing, will remain on the top; but the converse condition is impossible. This is because pure water at 40°F. is denser and heavier than water at any other temperature.

The same statement is true of air if instead of temperature in the usual sense we use the very convenient term suggested by Von Bezold, "potential temperature." "Potential temperature" is the temperature the air would have after its pressure had been altered to some standard pressure. Thus air of higher "potential temperature" can not lie under air of lower "potential temperature," The condition is unstable, like an egg standing upright on one end; but if the bottom air is only so much warmer than the upper that on rising it will be cooled below the temperature of the upper layer on reaching the same height, then the condition is stable like an egg lying on its side.

In what follows "potential temperature" will mean the temperature of dry air referred to the standard pressure of 300 mbar. (225 mm. or 8.86 inches of mercury); "warm" and "cold" will be used not as absolute terms but with reference to the usual temperature, just as we should call a day with a mean temperature of 50° F. cold if it happened in July, but very warm if it happened in January; and temperatures will be given in the centigrade absolute scale, on which 273°A. is the freezing point of water.

The change from actual to "potential temperatures" is troublesome when large changes of pressure are concerned, since logarithmic tables are required. The connection is

$$\log \frac{T}{T_0} = 0.29 \log \frac{P}{P_0}$$

where T_0 is the "potential temperature" and T the actual, P_0 the standard pressure and P the actual. But when the changes are small we have, by differentiating, $\partial T/T = 0.29\ \partial P/P$ where T and P are the existing temperature and pressure and ∂T and ∂P are small corresponding changes.

In recent years we have obtained fairly accurate knowledge of the temperature of the air up to 20 kilometers (12½ miles) over Europe and some knowledge of the conditions in tropical regions. The mean values are given in Table 1. The values for London are based on some 150 observations made during the years 1908–1914 in the neighborhood of London; they probably give correct means for those years within 1° or, at the outside, 2°C., and they agree within one or two degrees with the results obtained on the continent of Europe. The values for the Equator are more uncertain, observations there are too scanty as yet to give a mean that is reliable within some 5°C., but all the observations that have been made are consistent in this, namely, that extreme cold is met with at heights of 16 kilometers and over in the equatorial regions, and no doubt whatever remains that by far the lowest natural air temperature that has yet been measured is found high up in those regions.

The third column gives the temperature that air at any point would have if brought adiabatically under a pressure of 300 millibars. The temperatures would be different if some other standard of pressure were adopted, but the figures show two things: Where there is a large change of "potential temperature" with a small change of height the air is in a very stable condition and will strongly resist any vertical displacement. Secondly, if air in the process of circulation passes from A to B, then if the "potential temperature" at A is higher than at B, the air must lose heat (not necessarily temperature) in its passage, and conversely if the "potential temperature" is highest at B it must gain heat in its passage.

TABLE 1.—*Table of mean pressures and temperatures over London and the Equator.*

Height.	London.			Equator.		
	P.	*T.*	Pot. temp.	*P.*	*T.*	Pot. temp.
Km.	*Mbar.*	°*A.*	°*A.*	*Mbar.*	°*A.*	°*A.*
20	55	219	358	53	193?	319
19	64	219	343	63	193?	303
18	75	219	328	75	193?	288
17	87	219	314	90	193	274
16	102	219	300	107	195	263
15	119	219	287	128	198	254
14	139	219	274	152	203	247
13	163	219	262	178	211	246
12	190	219	250	209	219	244
11	223	220	240	244	227	242
10	260	222	232	283	235	239
9	303	227	227	327	243	237
8	352	233	222	376	251	235
7	407	241	230	430	258	232
6	468	248	218	491	265	230
5	537	255	215	558	272	227
4	612	262	212	632	279	224
3	697	268	209	713	285	222
2	791	273	206	803	290	218
1	897	278	202	903	295	214
0	1,013	282	198	1,013	300	210

There are two further points to be borne in mind with regard to the "potential temperatures." These temperatures are calculated on the assumption that the air is dry. This is not the case. The presence of vapor, however, does not matter until it begins to condense, but as soon as condensation commences latent heat is set free and the 0.29 in the formula giving the "potential temperature" ceases to be applicable. The value that replaces it depends upon the temperature.

The common rule given with regard to adiabatic heating and cooling is that rising air falls 10°C. in temperature for each kilometer that it rises and rises 10°C. for each kilometer that the air falls. This is true for short distances, but not for large. The rule is founded on the supposition that the special temperature conditions found in the air are such that the "potential temperature" is the same at each height. How widely this differs from the actual state of affairs is shown by the figures. The result is that for heights exceeding a few kilometers the allowance, 10°C. per kilometer, is too great, thus applying the formula $\log P/P_0 = 0.29 \log T/T_0$, and starting, from the ground level at London with a temperature of 282°A. the correct value at 10 kilometers is 190°A. and at 20 kilometers it is 120°A., instead of 182°A. and 82°A., which would be given by the 10° per kilometer rule.

It is perhaps hardly desirable to base any remarks on the general circulation on such localized observations as those collected in the table; but yet, on the other hand, all the observations on the temperature of the upper air that are available are more or less consistent with each other. It is only over Europe that the observations are sufficiently numerous to give the conditions in any detail, but the results obtained from North America, Australia, and elsewhere do not contradict any conclusions that can be drawn from the observations over Europe. Over Europe the effect of change of latitude is quite apparent, but no certain difference between east and west can be discovered. All available observations support the general rule that where the lower air strata are warm the upper are cold, so that while near the surface as one goes toward the Equator the temperatures rise, but above 12 kilometers or so the converse conditions are found and it is the polar regions that are warm and the equatorial regions that are cold.

The differences of pressure at the same level are a striking point in Table 1. If two columns of air of different temperatures but the same pressure at the bottom, could stand side by side the greatest difference of pressure between them would be at the height of the homogeneous atmosphere, i. e., at a height of from 8 to 9 kilometers. Thus in Table 1 from 5 to 11 kilometers, differences of pressure of over 20 millibars are shown between the Equator and latitude 52° N. It is no great wonder, therefore, that the strongest winds should be found at these heights. At 18 kilometers the pressures are found to be the same. These pressures are of course calculated from the surface pressure and the observed temperatures, and we can not be sufficiently certain of the mean temperatures over the Equator to be sure that the pressures given in Table 1 are correct; but it is certain that above 10 kilometers the differences rapidly decrease and almost certain that somewhere between 15 and 20 kilometers there is a level where the difference is nothing.

It has been pointed out already that, owing to the earth's rotation, a wind does not blow direct from the high to the low pressure, indeed it is obvious that if such were the case the low pressure area would very rapidly be filled up by the air entering it, but it blows at right angles to the pressure gradient. Thus the decreasing value of the pressure at the cirrus level, as the latitude increases, will account for the westerly winds at that level. Also since the journey from the Equator to latitude 50° is a long one, there is ample time for a wind starting as a south wind to be turned into a west wind.

The prevailing winds at that level in temperate latitudes are known to have a westerly component, but it is equally certain that westerly winds are not always blowing, at least over Europe, for many balloons have been sent up and have fallen without encountering any drift from the west. It may be of interest to state here that over Europe the "center" of the falling place of balloons that reach from 12 to 20 kilometers height, is between east-southeast and southeast of the starting point, thus showing that over Europe there is a drift from the north as well as from the west. This is probably local, but since the rate of ascent of a balloon is much the same at all heights the mean drift must refer to height and not to the mass of the air. If, therefore, the drift in the lower strata is from the south, the balloons may still fall to the south without proving that the drift of the mass of the atmosphere is from the north, for half of [the mass of] the whole atmosphere lies below 6 kilometers and nearly three quarters lies below 10 kilometers.

If we average the temperatures (in Table 1) with regard to mass we find 250°A. for the mean in the neighborhood of London and 264°A. for the Equator. Taking the atmosphere as a whole it is as a matter of course continually losing heat by radiation into space, it is also being warmed by solar radiation, direct and indirect, and by contact with the earth or sea already heated by the sun. Since its temperature remains unchanged to any appreciable amount from year to year it must lose by radiation just as much heat as it receives. We may assume, probably without error, that the amount of heat the atmosphere radiates out into space varies as the fourth power of the absolute temperature in accordance with Stefan's law and if we take 255°A. as the mean temperature that is maintained under the average solar radiation, we can calculate what the mean should be under any other amount of solar radiation. In Hann's well-known book the solar radiation at the Equator is given as 350°A. in comparison with 240°A. in latitude 50°. Were there no circulation and were the temperature at each place dependent only on the ratio between the values of the radiation, the ratio of the temperatures between the Equator and latitude 50° should be the fourth root of 240/350 which is very nearly 91:100, and the ratio between 250 and 264 is a little under 95:100. The temperature difference is therefore less than the difference in radiation would lead us to expect and the equalization must be produced by the circulation. If the figures were sufficiently trustworthy one might get an idea of the interchange of air, but not only is the 264°A. subject to a large casual error, but also the 250°A. for Europe, though reliable, only refers to a small part of the latitude circle.

Knowing with fair accuracy the value of the solar constant, the amount of heat that the whole atmosphere loses per day can be estimated with some accuracy. The doubtful point is the amount of solar heat that is reflected back without being directly or indirectly absorbed by the air. The result is that the fall of temperature per day on account of "out" radiation alone is from two to three degrees. This is the average value; it would be more near the Equator, less near the pole.

The temperatures that are given in Table 1 are very interesting but they are difficult to explain. Before observations were set on foot no one would have expected that the lowest natural temperature that mankind has measured would be found at some 10 miles height over the Equator, yet so it is. The actual mean value, 193°A. (−180°C., or −112°F.), may be doubtful, but the value is certainly far below that found at the same height in temperate latitudes. The highest mean [upper air] temperature is given by Petrograd (Pavlovsk), the station of highest latitude from which regular observations are obtainable. It seems to me that there is one and only one feasible explanation. The low temperature must be due to the general ascent of the air, and it must occur in spite of and not in obedience to the radiative conditions. Radiant energy is most intense at the Equator, both direct and indirect. The earth and lower layers of air are warmer and the radiation they send upward is more intense. It is utterly impossible that this can be checked by a veil of cirrus or other cloud, for if checked it can only be by being absorbed or reflected; if reflected it must raise the lower temperature still more; if absorbed it must raise the temperature of the absorbing body until the latter gets so hot that it radiates outward as much heat as it is receiving from below.[3] Thus inevitably air at 16 kilometers over the Equator must, so far as radiation only is concerned, with intense solar radiation coming from above and hot damp radiating layers below, be warmer than air at the same level in the winter over the plains of northern Russia, with no appreciable solar radiation from above and a surface of snow below having a temperature not much above the freezing point of mercury. Yet the air high up over the snow-covered plain is about 30°C. (54°F.) warmer than that over the rank steaming jungle of the Tropics.

The fact can be explained if we suppose a steady though slow rise of air in the Tropics and a corresponding fall in regions nearer the poles. There is overwhelming evidence to show that away from the earth's surface the local distribution of temperature is governed far more by the distribution of pressure, which alters the temperature directly and also by inducing vertical motion, than by radiation. This evidence will be given later. The question of the radiation of the atmosphere is a very difficult one. It has been shown that the atmosphere, taken as a whole, at a temperature of about 255°A., would lose by "out" radiation about 2.5°C. per day. It may be inferred that if the atmosphere as a whole had a temperature of 280°A., say, it would lose $2.5° \times (280/255)^4$ or nearly 4°C. per day, and if a temperature of 230°A., it would lose about 1.3° per day. But it is not safe to infer that a small mass of air at 280°A. placed anywhere loses at the rate of 4°C. per day (of course we are taking no account now of the energy that is absorbed) because the absorbing and radiating power of air depends very greatly on the amount of water vapor and carbonic acid present. Warm air in general, even in places like the Sahara, has plenty of water vapor, cold air can contain very little, hence warm air possesses a power of radiation which cold air does not possess and the question becomes very complicated. Added to this there is the radiation from clouds, of which we know very little. A heavy rainfall puts an enormous amount of heat into the atmosphere; this heat does not seem to alter the temperature much and it is hard to see what becomes of it unless it be dissipated by radiation from the top of the storm clouds that cover the rainy area.

One point, however, is fairly clear. The mutual radiation between different parts of the atmosphere must tend to equality of temperature throughout. Air, like all other substances, can absorb just those wave-lengths which it can radiate, both processes for dry air at any rate are slow, but perhaps all air possesses sufficient moisture to render radiation efficient. We know that the atmosphere is able to absorb a large proportion of the solar rays.

The air is actually warmest at the bottom and there are two separate ways in which this may be brought about. Both the temperatures of the sea and of the ground are higher than that of the air lying just above them, at least this rule holds for the warm parts of the earth and for the summers of temperate latitudes. This higher temperature is produced by the solar radiation that gets through the atmosphere and reaches the ground and thus affords to the air a source of heat at the bottom. The result must be a temperature gradient from below upward, but it is not easy to estimate what the magnitude of this gradient ought to be. The other possible explanation will be discussed later.

The actual figures in the columns (Table 1) showing "potential temperatures" are of no importance because they depend on the standard pressure chosen, which is quite arbitrary. The difference between two "potential temperatures" is significant so long as we do not depart too far from the height corresponding to the standard pressure, in this case about 9 kilometers. If the air at any point were in neutral equilibrium, the "potential temperature" would not change with the height. The fact that the "potential temperature" everywhere increases with the height proves the stability of the atmosphere. Equal "potential temperatures" would correspond to an ocean of water, pure or of uniform salinity and of equal temperature throughout. Ascending and descending currents could be set up with ease in such an ocean. The actual conditions correspond to an ocean having layers of different liquids, the heavy liquids being at the bottom, the light at the top, and the different density of each layer is shown roughly by the reciprocal of the "potential temperature." Such an ocean would permit of horizontal currents being set up without much trouble, but it would resist vertical circulation.

The "potential temperatures" of Table 1 are calculated on the assumption that we are dealing with air in which water is not being condensed. For parts in which rain is being formed the 0.29 of the formula is too high and the "potential temperatures" would be nearly equal for successive kilometers. In such parts, therefore, the stability need not be large. But ascending air in one place is of necessity accompanied by descending air somewhere else and for descending air the "potential temperatures" of the table are correct. Also the parts in which rain at any moment is being formed are, in comparison with the whole atmosphere, of very limited extent, hence the average condition is one of considerable stability and vertical motion will be strongly resisted.

A second point about the "potential temperatures" is this. It has been already pointed out. If air passes from one place A to another B, and the "potential temperature" of B is above that of A, then on the whole during the passage the air must receive more heat than it loses. For air that does not touch the ground the sources of heat are only two, viz, heat set free by condensing water vapor and heat received by radiation from bodies warmer than itself. The converse holds, but the loss of heat can only be by radiation to colder bodies or to space.

It follows that all descending air is losing heat. This is readily explained, for descending air becomes warmer than its surroundings since the average temperature gradient is not equal to the adiabatic gradient, and therefore it loses heat by radiation to the neighboring air.

Ascending air is gaining heat. The explanation is similar to, but the converse of, that given above, but in addition in the lower strata ascending air produces rain and the air receives heat from the latent heat of

[3] Compare in this connection the exposition by *W. J. Humphreys* in Bulletin of Mount Weather Observatory, 1911, v. 4, pt. 3, p. 130-131.—C. A., jr.

condensation. This fully explains the greater rise of "potential temperature" in the lower strata.

Air passing horizontally near the surface is gaining heat when going toward the Equator. This is natural enough, for a north wind is naturally cold and is being warmed by its surroundings. The converse also holds.

The most interesting case is that of the horizontal passage of air north and south at great heights. The temperatures above 17 kilometers at the Equator must be accepted with great reservation, since observations are very scarce, but from about 12 to 17 kilometers the equatorial potential temperatures are certainly much below the others. The pressures are greater also, so that the flow of air should be toward the north. If the flow is toward the north the air is gaining heat and it seems to me that from its situation it ought to be doing so. It is very cold and therefore is not itself radiating much either upward or downward, and it has passing through it outward the whole radiant energy which is being sent outward by the earth and by the four-fifths of the atmosphere which lie below it, and much of which is at a far higher temperature. It is likely to be a long time before any direct observations can show which way the drift is, for a very slow motion will suffice to explain the low actual temperatures over the Equator. The general motion is from the west, and it is a question whether on the balance over the whole zone the direction is a few degrees north or a few degrees south of west. The ascending current over the Equator is certainly slow, a hundred feet a day, perhaps, or something of that sort; otherwise the temperature gradient would be steeper; and the air carried up by so feeble a draft could readily escape north and south without detection, even if we had as precise observations of the upper winds as we have of the surface winds.

Reverting to the actual temperatures, it is noteworthy how similar the temperature gradient is in the two localities. It begins at about 5°C. per km., then after the cloud level, excluding cirrus, is passed it increases to 7° or 8° per km., up to the point at which it stops altogether. This point is higher the lower the latitude. Observations from all parts of the world show the same tendency, except that where the surface temperature is very low the gradient in the lower strata is absent or reversed.

It seems to me likely that this special form of gradient is a sort of compromise between two opposite tendencies. It is obvious that if the air could be thoroughly mixed up, the adiabatic gradient would prevail throughout, for the mixing will make equal potential temperatures, just as stirring the water in a bath makes a uniform temperature. Now, the winds, however they may be caused, must do a certain amount of mixing and hence must raise the bottom temperature and lower the top; and unless there were something else to reverse the result the process would go on until the gradient were adiabatic. But radiation checks the result, I believe, for in my opinion the tendency of radiation is toward a uniform temperature or to a small gradient. The argument seems clear that the winds alone must make an adiabatic gradient, since they do not do so something must interfere with the process and that something can be nothing else save radiation. It is significant, too, that observation shows that the wind falls off rapidly at or about the point where the temperature gradient ceases.

The statement previously made that upper-air temperatures depend on the pressure distribution rather than on radiation is based on the following facts. So many observations have been made over Europe, ranging from Pavia in the south to Petrograd in the north and from Ireland in the west to Russia in the east, that we have quite a good knowledge of the conditions both in summer and winter and in times of cyclonic and of anticyclonic weather. Over the British Isles solar radiation in summer is about three times as great as in winter and the temperature of the air up to 10 kilometers is in general 12°C. (21°F.) warmer. Nothwithstanding this, for the purpose of knowing the most likely temperature of the upper air, it is more important to know the height of the barometer than to know the time of the year, for after the first kilometer or two is passed the air in a well-marked cyclone will be colder in summer at from 3 to 7 kilometers than it is at the same height in winter in anticyclonic weather; and above 12 kilometers the converse will hold. The same rule holds for Europe generally, but cyclonic conditions there are much less frequent. Moreover, it is not the pressure at sea level that is important, but the pressure at about 8 or 9 kilometers. The temperatures both above and below follow the variations of pressure at 9 kilometers with the utmost precision, and if a chart showing pressures at 9 kilometers could be given it would be easier to draw a chart of temperatures at 5 or, to a less extent, at 15 kilometers than it is to draw a chart of wind forces and directions from isobars on an ordinary chart. The pressure at about that height seems to dominate all the other elements.

FORECASTING THUNDERSTORMS.[1]

By GABRIEL GUILBERT.

[Dated Aug. 3, 1912.]

1. Wireless telegraph installations are known to possess the curious property of recording [indicating] the electrical manifestations produced in their neighborhood and even at very great distances.

Thus, on March 4, 1912, a formidable thunderstorm accompanied by trombes descended upon Calvados [Département Caen] at about 19h and a meteorologist of the Lyon observatory, M. Flajolet, simultaneously observed his wireless apparatus to record powerful distant phenomena.

It was but a step from this observation to the thought that it would be possible to announce for a given point, the approach of a distant thunderstorm indicated by the wireless outfit, and physicists such as M. Turpain of Poitiers have attempted these predictions.

So far it has not been possible, unfortunately, to forecast the direction of these distant thunderstorms. Thus, in the case of the squall of March 4, 1912, the apparatus at Lyon did indeed record the existence of a storm, but could not indicate whether or no the storm was approaching the observatory. As a matter of fact it was traveling toward the NNE. part of France and was moving away from Lyon. The wireless telegraph remained and will remain mute, powerless, on this essential point as well as on the speed of the storm.

Certain squally clouds are indeed either very slow moving or very rapid; they may move at the rate of 20 or of 100 km.-hr.; wireless telegraphy knows nothing as to that and can not know anything.

On the other hand, certain meteorologists believe that they can notice that the wireless apparatus also reacts to phenomena quite other than thunderstorms; there is thus

[1] *Translated from* Association Française pour l'avancement des sciences, Compte rendu, 41me sess., Nîmes, 1912. (Paris, 1913), p. 296-304.—C. A., jr